

Price-Concentration Studies: There You Go Again

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Abstract

Price-concentration studies are important because they are widely seen as supporting the standard prediction that high seller concentration tends to harm consumers and because both antitrust agencies use them in assessing horizontal mergers. But price-concentration studies are severely flawed. In industries in which sellers compete on quality and amenities, a positive price-concentration relation could result, not from coordinated effects, but from competitive superiority. Since such non-price competition is ubiquitous and is usually intense, this poses a significant problem of interpretation. Further, non-price competition cannot be controlled for statistically, as is frequently assumed. Even if sellers compete exclusively on price, price-concentration studies are still severely flawed. Differences in market size could induce a positive price-concentration relationship, but as with competitive superiority, a price-concentration relationship resulting from this cause does not imply consumers are being harmed. Market size differences may well account for the observations that the FTC attributed to non-competitive behavior in *Staples*. The paper concludes with some suggestions for improving price-concentration studies and for further research.

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Stylized Fact 5.1: In cross-section comparisons involving markets in the same industry, seller concentration is positive related to the level of price. . . . The relation between concentration and price seems much more robust statistically than that between concentration and profitability. Since studies of price have fewer obvious weaknesses than studies of profitability, Stylized Fact 5.1 seems to provide the best evidence in support of the concentration-collusion hypothesis [footnote omitted].

Richard Schmalensee (1989, p. 988)

In this study, and several others in this book, it is clear that concentration does raise price. The basic hypothesis of oligopoly theory has lots of support. Our two-decade digression on concentration and profits and our subsequent 13 years of doubt were unnecessary.

Ronald H. Koller II and Leonard W. Weiss (1989, pp. 36-37)

Tacit collusion, often described as price followship, most certainly does occur in concentrated markets. How otherwise would one observe a positive relationship between concentration and price levels?

Ronald W. Cotterill (1990, pp. 197-198)

A more direct approach is to test the relationships between market structure and selling prices directly (Weiss). . . . Here, the relationship of concentration to price is uniformly positive, which tends to dismiss the efficiency hypothesis as empirically groundless.

John M. Connor (1990, p. 1222)

My overall assessment is that the concentration-price studies clearly establish the association between industry structure and performance in a manner not subject to the Chicago-school or any other criticism of profits studies. For that reason, I myself ceased emphasizing concentration-profits relationships in my teaching and research some years ago, focusing instead on price studies.

John E. Kwoka, Jr. (1990, p. 48)

Regarding the qualitative adequacy of these studies, I see no substantial problems with them. As Weiss argues, for a given product that is sold in numerous independent markets, testing the effect of concentration on price is a much simpler research problem than testing the effect of concentration on profits across product markets. The control and measurement problems are much less troublesome.

Richard E. Caves (1990, p. 52)

A positive correlation between concentration and profits also is consistent with perfectly competitive behavior in all local markets. For example, higher profits

in the more concentrated markets may reflect lower costs (efficient-structure hypothesis) rather than noncompetitive behavior, as discussed in Smirlock, Gilligan, and Marshall (1984) and Berger and Hannan (1989). The latter employ information on retail deposit rates (*prices*) to test the SCP relationship in local banking markets. This approach excludes the efficient-structure hypothesis that can explain a positive relationship between *profits* and concentration.

Paul S. Calem and Gerald A. Carlino (1991, p. 268)

It turns out that the market-power theory deserves our greatest respect. *Price levels are positively associated with market concentration measured in a variety of ways* (e.g., four-firm concentration ratio or H index or a shrinking number of firms). . . . Thus despite the difficulties, many dozens of empirical studies have been conducted, and all but a few of them show a positive relationship between concentration and price level. They therefore support the market-power hypothesis and refute the market-efficiency alternative [footnote omitted].

Douglas F. Greer (1992, p. 308)

The methodology used in these price-concentration studies differs from that used in the earlier profits-concentration literature. . . . their inferences about competition are less likely to be biased by the across-industry differences in technology that are difficult to fully control for. Direct studies of output prices also avoid the substantial problems of accurately measuring profits [footnote omitted].

Timothy Dunne and Mark J. Roberts (1992, p. 13)

Other studies have focused instead on the correlation between concentration and price levels. . . . These studies are less likely to confuse market power with efficiency because any extra efficiency among leading firms in a competitive market would tend to show up as lower, not higher, prices.

Sherill Shaffer (1994, p.7)

Second, and most widely done, has been the use of prices instead of profits to analyze the performance-concentration relationship. This methodology largely rules out the possibility that the S-P relationship is due to efficiency.

Stephen A. Rhoades (1996, p. 358)

. . . if there appears to be a positive relationship between price and concentration (i.e. price is systematically higher where concentration is higher), this suggests that the merger may lead to higher prices.

Lexecon Ltd. (1997, p. 1)

On the empirical side there is a growing list of studies that finds positive relationships between market concentration or firm market shares and prices (e.g. Weiss, 1989; Evans and Kessides, 1994). These studies remove much of the ambiguity of traditional structure performance studies over whether

concentration (market share) and profit rate correlations are driven by efficiency or market power differences. If high concentration and market shares were due to greater efficiency we should expect lower prices in concentrated markets, which is not what we observe.

Dennis C. Mueller (1997, p. 701)

Studies of a relationship between market concentration and prices largely avoid the problem of efficiency effects because greater efficiency would be associated with lower costs (and therefore more favorable prices to consumers), while greater market power would move prices in the opposite direction.

Katerina Simons and Joanna Stavins (1998, p. 17)

The key factor supporting a likelihood of coordinated interaction is the level of concentration. Economic studies have suggested that price and concentration in grocery markets are related [footnote omitted]. As Areeda and Hovenkamp observe, in food retailing “significant increases in concentration . . . led to significant increases in price notwithstanding easy entry [footnote omitted].” Thus, the higher the level of concentration, the higher is the risk that an increase in concentration will lead to higher prices through collusion.

David A. Balto (1999, p. 12)

By comparing the same products across distinct markets, researchers can be more confident that variations in price are due to variations in competition, rather than variations in accounting, or other factors.

David Besanko, *et. al.* (2000, p. 258)

The late Leonard Weiss in 1989 collected all the studies he could find concerning the comparative impact of concentration on price [footnote omitted]. The overwhelmingly consistent outcome was that prices were higher in concentrated markets even though profits were not consistently higher. The implication of these results is that concentrated markets impose costs on consumers and suppliers who sell into such markets, but such markets are not more efficient. . . . there is no social gain. There is only social cost.”

Peter C. Carstensen (2000, p. 536)

There is, for example, recent literature which suggests that food prices are higher in more highly concentrated grocery markets [footnote omitted]. Perhaps in part for this reason, merger enforcement has become more vigorous in recent years.

Robert H. Lande (2001, p. ?)

One type of concentration-price study uses data from separate geographic markets in the same industry. . . . These studies all show strong evidence of a positive relationship between concentration and the level of price. Given that price-concentration studies have fewer problems than profitability-concentration studies, these robust results provide strong support for the

concentration-collusion hypothesis.

Don E. Waldman and Elizabeth J. Jensen (2001, pp. 516-517)

. . . several studies of differing industries using price to measure performance suggest that increasing concentration may indeed lead to higher prices. The price evidence, while not without its own weaknesses, is probably more reliable than is the profit-based evidence.

Paul A. Pautler (2001, p. 54)

Moreover, there is now a large literature on the relationship between concentration and price within particular industries [footnote omitted], and this literature, although criticized [footnote omitted], has not been discredited.

Gregory J. Werden (2001, p. 64)

Second, the empirical evidence is consistent with a positive relationship between market concentration and price. We readily acknowledge that the studies finding such a relationship are not perfect. They may not always define markets properly, or adequately account for the reverse effect of price on concentration, for example. And, it certainly is true that collusion does not occur in every highly concentrated market, while collusion does occur in some markets that are not highly concentrated. Still, Professor Richard Schmalensee's 1989 summary remains a proper interpretation of the empirical studies . . ."

Jonathan B. Baker and Steven C. Salop (2001, pp. 8-9)

Sometimes, we have an industry that has multiple markets within that industry and they have different firm organizations or different concentrations, for example, across the different markets. In these circumstances, doing a cross-sectional study can be a very useful way of understanding whether, for instance, we should be worried about a merger based on whether concentration appears to matter.

Michael Katz (2002, p. 5)

A large number of empirical studies—in industries ranging from banking services to cement, and from offshore oil and timber auctions to newspaper advertising—have found that prices tend to be higher on average in industries with higher levels of concentration or fewer suppliers [footnote omitted]. This empirical regularity is consistent with concentration fostering coordination (as well as with some unilateral effects theories).

Andrew R. Dick (2002, p. 5)

There you go again.

Ronald W. Reagan to Jimmy Carter, presidential candidates' debate, 1980

1. Introduction: why examine price-concentration studies?

Let's start with two propositions that seem uncontroversial. First: the specific Herfindahl numbers used in the Merger Guidelines—and the Guidelines generally—should have some empirical basis. Michael Whinston makes a representative statement:

All of the foregoing discussion has focused on a *prospective* analysis of horizontal mergers. It is natural to ask, however, what we know, looking *retrospectively*, about their *actual* effects. Such analyses can be useful for at least two reasons. First, they can guide our priors about the likelihood of mergers being anticompetitive or efficiency enhancing (ideally, as a function of their characteristics). Second, we can use this information to assess how well various methods of prospective merger analysis perform . . .” (Whinston, 2003, pp. 42-43)

Several observers have noted that since their beginning, the Guidelines have been influenced, in fact, by empirical research (White, 1987, p. 17; Schmalensee, 1987, p. 49; Kwoka, 2003, p. 5). The reason why is clear. Economic theory does not make unconditional predictions as to what levels of market concentration and firm market share will lower consumer welfare (Fisher, 1987, p. 30; Hay and Werden, 1993, p. 173; Kolasky, 2002b, p. 3; Baker, 2002, p. 152).¹

Second: we have little empirical evidence that bears directly on how horizontal mergers affect consumer welfare. Here are three representative statements:

It's really quite surprising to me that this would be such an important policy arena and we would have so little follow-up on the effects of mergers. (Michael Whinston in U. S. Federal Trade Commission, 2001, p. 21)

Providing empirical evidence on the question of the effects of antitrust policy is a commendable objective. Along with many other observers, this author has long noted the absence of systematic evidence or even many good case studies that establish the effects. (Kwoka, 2003, p. 1)

The sad truth is that despite endless calls for empirical study of the effects of mergers and of antitrust enforcement of section 7, there is very little empirical evidence demonstrating the effects of mergers (or enjoined mergers) on consumer welfare. (Gurrea and Owen, 2003, p. 21)²

¹ Even if the theory were more helpful, it would still be desirable to assess how well the antitrust agencies have applied the theory to the facts in specific cases. For instance, the theory indicates that it is vital to assess how quickly and how effectively entry would occur in response to non-competitive behavior (Fisher, 1987, p. 35; Note, 1999, pp. 2425-26; Baker, 1997, p. 181). How well the agencies do this can only be assessed empirically. See, for example, Christopher Grengs who argues (Grengs, forthcoming) that the FTC drastically underestimated the prospects for effective entry in *Staples*.

² Gregory Werden, although more sanguine about the currently available evidence, writes (2003, p. 5), “There is a need for additional empirical evidence on the price effects of criminally prosecuted cartels and mergers . . .”

As a result, empirical support for horizontal merger policy currently rests on several types of indirect evidence. This evidence includes merger simulations, stock market event studies, studies of legal cartels, and experimental evidence. (See Pautler, 2001 for a thorough survey; for helpful discussions also see Werden, 2003; Baker, 2003; and Whinston 2003.)

And the indirect evidence includes price-concentration studies. Each price-concentration study examines an industry, such as grocery retailing or retail banking, in which the economic markets are local. The study then investigates how price varies across local markets as a function of seller concentration.³

Price-concentration studies are important to industrial organization economists and to the antitrust agencies for at least three reasons.

1. They are widely thought to improve upon interindustry profit-concentration studies. Profit-concentration studies have two serious flaws. They are plagued with measurement problems. For a number of reasons accounting profits are, at best, poor proxies for economic profits. Also, the huge differences across industries in demand and supply conditions are hard to control for econometrically (Hall, Schmitz, and Cothorn, 1979; Weiss, 1989a, pp. 5-7; Werden, 1991, pp. 6-7; Dunne and Roberts, 1992, p. 13).

Profit-concentration studies are also difficult to interpret. If highly concentrated industries earn higher profits than less concentrated industries, it could be because high concentration fosters collusion, either explicit or tacit. But, as is now well known, profits and concentration can be positively correlated for another reason, a reason having an entirely different implication for consumer welfare. Suppose a small number of firms in an industry develop products superior to their rivals. Those firms would gain share, concentrating the industry. They would also earn, at least for a time, abnormally high profits. Profits could well be positively correlated with seller concentration because of the “efficiency” or “competitive superiority” of the leading firms. If so, higher seller concentration does not decrease consumer welfare, it actually increases it (Demsetz, 1973).

Price-concentration studies are widely thought to overcome these problems. (See the two dozen quotes that begin this paper.) Prices are supposedly much more easily observed than economic profits. At the very least, prices are not subject to the many accounting conventions that greatly complicate studying profits. The differences in demand and supply conditions within a single industry examined by a price-concentration study should be much smaller than the differences between industries that must be controlled in profit-concentration studies. Finally, the interpretation problem seems to disappear. If seller concentration raises prices, we have a clear reason to oppose seller concentration: how could higher prices possibly result from “competitive superiority”? How could higher prices possibly benefit consumers?

³ Seminal research in this literature is Weiss, 1989b. Greer, 1992, pp. 310-311 presents an exhaustive list of early price-concentration studies. Pautler, 2001, pp. 42-47 discusses some of more recent research. Note that some of these studies pool cross-section data with data over time. I discuss aspects of the pooled studies in part III below.

In articles, monographs, and textbooks, price-concentration studies are therefore cited to justify more intense scrutiny of concentrated markets. They are also cited to justify the stricter review of horizontal mergers in more concentrated markets. For example, Jonathan B. Baker wrote recently that price-concentration studies are an important foundation for the “structural presumption” (Baker, 2002, pp. 152-53).⁴ And price-concentration research is used to explain why challenging horizontal mergers in concentrated markets benefits the public.⁵

2. Price concentration studies have been used, and are being used, by the DOJ and the FTC. Michael Winston writes (2003, p. 28) that price-concentration studies “. . . are the most commonly used econometric technique employed in current merger cases.” Ronald W. Cotterill wrote (1993, p. 224), “. . . the Federal Trade Commission has relied upon these studies in its challenge or negotiation of consent decrees in some merger matters, most notably the National Tea Applebaum merger (1979), the Safeway sale of its El Paso division to Furrs (1987) and the acquisition of Grand Union by Miller Tabak and Hirsch the investment holding company that owns P&C Markets (1989).” Michael Moiseyev (1997) wrote that a price-concentration study played a role in the FTC’s decision to challenge a major mid-90s merger in the drug retailing industry. William Kolasky states (2002a, p. 11) that price-concentration studies are one of the seven “more common types of empirical work” done by the Economic Analysis Group of DOJ’s Antitrust Division. Finally, Baker and Rubinfeld observe (1999, p. 405):

Reduced form price equations are the workhorse empirical methods for antitrust litigation for several reasons. First, they can provide evidence on a wide range of problems. Second, they are among the most straightforward regression models to explain to generalist judges or juries with little or no background in statistical techniques. Third, they are among the least demanding in terms of data and computational difficulty.⁶

3. Conditions in the future are likely to make price-concentration studies even more attractive to academic economists and to the antitrust agencies. Most of the price-concentration studies examine retail industries. Many sectors of retailing are consolidating (Balto, 1999, p. 2), and will likely propose more mergers for the agencies to evaluate. That, combined with the increasing availability and detail of price data collected by firms, should make price-concentration studies ever more appealing.

Unfortunately, the vast majority of price-concentration studies do not provide useful guidance for antitrust policy. While it’s not hard to find reservations and warnings and critical comments about these studies, the depth of their problems seems to be neither widely nor fully appreciated. That those problems are serious, serious enough that the price-

⁴ Hay and Werden wrote (1993, p. 175), ““We suspect that objections to the concentration-profits studies have played a significant role in the relaxation of merger standards.” If so, it’s logical to assume that price-concentration studies are one reason why the antitrust agencies are now more actively opposing horizontal mergers. Lande (2001, p. ?) also suggests this.

⁵ See, for example, Passell 1997.

⁶ The most noted application, of course, is in the FTC’s case against *Staples* (*Federal Trade Commission v. Staples, Inc.*, 970 F. Supp. 1066.) I discuss *Staples* in part III below.

concentration studies probably should be discarded just like the profit-concentration studies have been, is what I argue in the rest of this paper.⁷

II. Price-concentration studies of industries in which sellers compete on quality and amenities have the same severe, fundamental problems as profit-concentration studies

Price-concentration studies should focus on industries in which the product is identical across markets and across sellers, such that sellers compete exclusively on price.⁸ But such markets are extremely rare. Even if the product is identical, conditions of sale almost never are. Competition on non-price attributes is generally intense and it is ubiquitous. Before discussing this, let's consider what it means for price-concentration studies. If non-price competition is important, price-concentration studies have the *exact same interpretation problem as profit-concentration studies*.

Suppose that one or two sellers in a particular market—call them “superior sellers”—develop products that are more appealing to consumers than their rivals' products. So appealing that consumers are quite willing to buy the superior firms' products even at higher prices. At least for a time, therefore, the superior sellers gain market share and the market becomes more concentrated. But prices are also higher in that particular market than in other, less concentrated markets. We would observe, therefore, a positive—maybe even quite large—correlation between price and concentration that indicates absolutely no harm to consumers and offers no absolutely guidance to antitrust. This serious complication of non-price competition was recognized in the original statement of the competitive superiority hypothesis (Demsetz, 1973, p. 4): “. . . if there is no single price upon which the industry agrees, but, rather a range of prices, then one firm can earn a higher rate of return if it produces a superior product and sells it at a higher price without thereby incurring proportionately higher costs; here, also, the firm that earns the higher rate of return can be judged to be more efficient because it delivers more value per dollar of cost incurred.”⁹

⁷ I reference many of the criticisms in my discussion below. But first a comment about previous criticisms. Most of these comments, read in context, can be fairly characterized as “perfunctory.” (Exceptions include Phillips, 1980; Bresnahan, 1989; Elzinga, 1989; Anderson, 1990; Fisher, 1991; Snyder, 1991; Hausman and Leonard, undated; and Carlton and Perloff, 2000.) Virtually all empirical work in economics has problems. After considering the problems in a particular line of research, observers have to decide, finally, whether the work is useful or not. Many observers clearly do feel that price-concentration studies are, despite the weaknesses they note, important and useful. If nothing else, the antitrust agencies' use of price-concentration research—proudly and with great fanfare in the case of *Staples*—suggests the high level of respect they command among many interested observers.

⁸ This has been noted by several observers. See, for examples, Werden 1991, p. 7 (“. . . the results of studies may be misleading if there are important differences in products not adequately controlled for and higher prices are associated with higher quality.”) and Pautler 2001, p. 42, who qualifies his support for price-concentration studies by writing, “If one can obtain transactions data for homogeneous product markets . . .”

⁹ Another way to say this is that price and concentration are both affected by a third factor, competitive superiority. This means that in regressions of price on concentration it is an error to treat concentration as exogenous. This has been noted previously, perhaps most forcefully by Carlton and Perloff, 2000, p. 259: “The more serious conceptual problem with many SCP studies is that the structural variables are not exogenous. . . .”

The authors and users of price-concentration studies are aware, of course, of non-price competition. How then do they propose to avoid this fundamental difficulty of interpretation? There seems to be two answers: 1) they can, and have, looked at markets for which non-price competition is unimportant, and 2) even if non-price competition is important, it can be controlled for by adding cost or quality variables to price-concentration regressions. I think both answers are unsatisfactory.

Consider the first response, that the problem can be avoided if we restrict our attention to certain products. Leonard Weiss wrote (1989a, p. 9), “Could high prices be read as an indication of superiority? Perhaps for wine in some sense, but surely not for cement.” Scherer and Ross (1990, pp. 439-40) echoed Weiss: “High prices might still indicate superiority of product or service quality. But this can be mitigated by focusing on industries where product differentiation opportunities are relatively limited, for example, by studying cement rather than wine or computers.” Berger and Hannan address the problem (1989, p. 292) by asserting that, at least in retail banking, it can’t exist: “quality differentials are fairly narrowly constrained by law,” so they “. . . can exclude from consideration the possibility that greater efficiency results in less favorable prices to consumers accompanied by more than compensating increases in quality.”

But the problem cannot be simply assumed or asserted away. Most of the existing price-concentration studies have examined retail markets. In retail markets non-price competition is common and important, and existing price-concentration studies have made little or no allowance for it. The reader doesn’t have to take my word for it. Ronald W. Cotterill wrote (1999, p. 101): “Nearly all prior studies of the structure price relationship assume that products are homogeneous and, consequently, do not test the Demsetz quality hypothesis [citations omitted].”

Let’s consider briefly some recent information on non-price competition in four industries, industries that account for most of the price-concentration studies we currently have: grocery retailing, retail banking, airline travel, and gasoline retailing.

Grocery retailing. The number of items stocked in chain grocery stores more than doubled from 1983 to 1995 (Nakamura, 1997, p. 23). Nakamura concludes (p. 10), “In brief, every statistic suggests that retail value added, measured by qualitative service dimensions, has been expanding.” A recently published study by Ratchford (2003) adds support. Ratchford defines two measures of grocery services. One is an unweighted average of the proportion of supermarkets offering each of the following services: deli, service bakery, service fish, service meat, film processing, dining, open on Sunday, catering, and pharmacy. The other is simply the average number of items per store. Both measures of grocery services increased over 1959 to 1995 at a compound annual growth rate of about 4.8%, almost three times the rate of

Even a correctly estimated relationship between performance and concentration is uninformative regarding causation. Concentration does not cause high profits; long-run barriers to entry do. These barriers lead to both high profits and high concentration [footnote omitted].” See also Schmalensee, 1990, p. 153; Anderson, 1990, p. 21; Snyder, 1991, p. 1206; Evans, Froeb, and Werden, 1993, p. 433; Baker and Rubinfeld, 1999, p. 405; Baker and Salop, 2001, p. 8; and Whinston, 2003, p. 29.

growth of physical output as calculated by BLS.

In recent work that is as yet unpublished, Paul B. Ellickson finds (pages 26, 27, and 32), “. . . the quality level of the top [supermarket] firms (as measured by store size and the number of store features) is almost twice that chosen by firms in the fringe. The top firms also operate many more stores, serve more markets, and are much more likely to vertically integrate into distribution. . . . All three measures of quality are significantly higher among the top 4 firms. These results suggest that membership in the ‘oligarchy’ requires providing a significantly different product than the fringe firms. . . . The preceding analysis demonstrates that competition among supermarkets yields a natural oligopoly of high quality firms that compete head to head in local markets.”

Finally, and of special interest for readers near Washington, D.C., consider Wegmans. This renowned grocery retailer will open a store near Dulles on Feb. 29, 2004. It will offer, among many other things, a coffee/cappuccino bar, cooking classes, a European bread bakery, organic produce, a patisserie, a Sushi bar, personalized cakes, and a wood-fired brick oven (http://www.wegmans.com/about/storeLocator/display.asp?store_nbr=7).

Banking. Petersen and Rajan report (1995, p. 407) that “Creditors are more likely to finance credit-constrained firms when credit markets are concentrated . . .” Berkstresser (2001) reports support for that finding.

For an October, 1995 American Bankers Association-Gallup poll, individual consumers were asked to state the most important reason why they maintained an account with a financial institution. The most popular reason, cited by 40%, was “convenience”; second was “friendly/good service”; and third, “a long-standing relationship.” “Good interest rates” was a weak fourth, at 11%. Businesses also ranked location first (Kwast, Starr-McCluer, and Wolken, 1997, pp. 14-15).

In a pair of unpublished papers, Astrid Dick (2001, 2002), using an approach similar to Ellickson’s supermarket study, and building on the idea of competition through endogenous sunk costs, reports evidence that larger-share banks offer higher quality products. Here are three brief passages in which she describes her findings:

For instance, while account fees have increased in some markets, many of the bank characteristics such as the number of branches in the local market, geographic expansion and size, have also increased, especially with the passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act which permitted nationwide branching as of 1997. Indeed, I find that out of 330 urban markets, 316 show increases in welfare for 1993-1999. This kind of exercise is at least suggestive of the bias that might arise in welfare inferences based solely on prices and concentration measures. (2001, p.2)

Dominant banks appear to provide more branches, which, in turn, have more employees, and they also tend to be more geographically diversified, have been around longer, and pay higher salaries to their employees [footnote omitted].

(2002, p. 15)

Furthermore, the paper sheds light on the empirical finding that larger banks charge significantly higher fees than smaller banks. The findings here indicate that dominant firms, which tend to be large banks, do charge higher fees yet invest more in quality. . . . it appears that quality is the result of banks' competitive investment in endogenous sunk costs . . . (2002, p. 23)

Air travel. Oliver Richard argues (2003, p. 908) that "To jointly incorporate valuation of ticket price and flight frequency requires a model with endogenous flight decisions." He specifies and estimates such a model, The model predicts that airline mergers generally lower consumer surplus, but that surplus "... increases in 11% of the sample markets. In those markets, consumers benefit despite the reduced competition, once increases in flight frequency following the merger are factored in, a result that derives from the comprehensiveness of the model."

Lee and Prado contend in a recent unpublished paper that previous research finding a "hub premium" do not determine clearly which customers are paying such a premium. They find that the premium is paid almost entirely by business travelers and that this affects how we should interpret the hub premium (2003, p. 4):

In general, we find that much of the observed 'hub premium' can be explained by fareclass mix. Indeed, controlling for fareclass reduced the hub premium for restricted coach passengers at 18 of the 19 hubs in our sample, often substantially. For premium passengers (those purchasing unrestricted coach, business and first class tickets), the results were more mixed, with the hub premium falling at 12 airports, but increasing at the other 7. Controlling for passenger mix reduced the average (across the 19 hubs in our analysis) hub premium from 20.8% to 11.9% for restricted coach passengers; for premium passengers, the average hub premium fell to 13.8%. In general, our findings are consistent with Ramsey pricing: leisure travelers have much more price-elastic demand than business travelers, and thus, the less convenient and lower quality connecting service offered by competing carriers is likely to have a greater disciplining effect on restricted coach fares than on premium fares for service to and from hubs [footnote omitted]."¹⁰

¹⁰ This raises an interesting issue that, as far as I'm aware, has not been completely resolved in the price-concentration literature: the joint demand for, and joint supply of, products in some retail industries. For instance, a grocery shopper is not interested *per se* in the price of a single item; the shopper is interested in the cost of the entire basket of goods, along with the retail services provided with that basket. Similarly, a bank customer may care a lot less about the interest rate on a given loan, then on his whole relationship with the bank, which may include the number and location of bank branches, the interest rate on deposits, extra services such as stock brokerage, etc. Sellers addressing these joint demands will typically incur large fixed costs. Who should pay these fixed costs? How do they affect the evaluation of higher prices for some components of the jointly provided bundle? A handful of papers about airline travel suggest that fares on individual flights must be viewed in the context of entire hub-and-spoke networks and that doing so can affect conclusions about consumer welfare. See Kleit, 1991; Kleit and Maynes, 1992; Spiller, 1989; Bittlingmayer, 1990; and Brueckner and Spiller, 1991. And though he is discussing a different issue, Baker (2003, p. 44) makes a similar point: "But in high fixed cost, low

Gasoline retailing. In an underappreciated paper, Png and Reitman (1994) examine competition among gasoline stations. The gasoline stations of different firms seem similar and the main product they sell—gasoline—seems very homogeneous.¹¹ But they compete on an important non-price dimension, waiting time. Some stations offer persistently lower prices but have smaller capacities and at them, customers wait longer; higher-priced stations offer shorter waiting times. They conclude (p. 633), “In service time competition, higher price does indeed mean higher product quality.”

Nor is gasoline, itself, as homogeneous as it might appear. Koch (undated, p. 5) argues as follows:

With many gasoline quality parameters regulated, the obvious question is if there is a difference among gasoline brands. The answer is a resounding yes. Differentiation is achieved through proprietary additive packages which contain anti-oxidants, metal deactivators, surfactants, deposit modifiers, corrosion inhibitors, and, of course, octane enhancers. In addition, gasoline retailers vary significantly in the level of care taken to prevent contamination; some companies have specific quality control procedures and special equipment to avoid contamination while others do not.

Non-price competition is not restricted to just these four industries. It is important in virtually all consumer-good industries and perhaps surprisingly, even in most producer-good industries. One simply has to look at how initially higher-priced audio CDs replaced vinyl records and at the success of Starbucks to see the importance of non-price competition. But we also have the statements of experts. Dennis C. Mueller observed (1986, p. 229):

The typical firm earning persistently high profits has a large market share in a differentiated product industry. If it is more efficient than its competitors, it is not because it produces the same product as they at lower costs, and sells it at lower prices. If anything, the price it charges probably exceeds that of its competitors for a product that is perceived to be superior along one or more product characteristic dimensions. Firms with high market shares tend to be more efficient than their competitors at developing their products, marketing them, and, perhaps most important of all, maintaining their image as a superior product. The successful firm is more efficient than its competitors in using nonprice modes of competition.

Carl Shapiro (1995):

To a greater or lesser degree, virtually all markets involve some element of product differentiation. Even in a classic homogeneous goods market—such as

marginal cost industries, perhaps including airlines, pharmaceuticals or software, some transactions may occur at prices levels in excess of incumbent seller marginal cost (as through price discrimination) even if entry is free, as across-the-board marginal cost pricing may not permit firms to cover their fixed investments [references omitted].”

¹¹ Interestingly for the topic at hand, they assert (p. 620), “Service stations—unlike banks, barbers, and supermarkets—sell fairly homogeneous products.”

the market for an agricultural commodity or for a specific chemical compound—producers often attempt to differentiate themselves based on product quality, reliability, or customer service.

Dennis W. Carlton (in U. S. Federal Trade Commission, 2001, p. 105):

. . . I was struck by how different prices were for what you would think about as pure homogeneous commodity goods.

And what that tells you is it's very hard to describe the product. You can describe the physical characteristics, but there are other characteristics like the speed of delivery, the timeliness of delivery, the reliability of delivery. All of these are very hard to measure, and account for very wide differences in prices.

Roberts and Supina (2000, p. 29):

While no manufactured product is perfectly homogeneous across sellers, we have chosen products to come as close as possible to that norm. Even in these cases, there exists substantial and persistent variation in output prices and markups across plants for most of the products we study.

Other observers make similar statements.¹²

The interpretational problem posed by non-price competition will probably get worse, not better. Non-price competition has been growing in breadth and intensity and will likely to continue to do so.¹³

By now, many readers may be wondering: isn't this what we have multiple regression analysis for? Can't adding quality or cost variables to the estimated regression model control for elements of non-price competition? In a word, no. Here are three reasons why.

1. It is serious challenge to identify, let alone measure, all the important forms of non-price competition. Baker and Rubinfeld (1999, p. 402) are optimistic that this problem can be overcome because "if variables unobservable to the econometrician are important in pricing, they must nevertheless be observable to executives who make pricing decisions. Accordingly, testimony and documentary evidence can be marshaled to improve the measurement of marginal cost or confirm that existing measurements are reasonable."¹⁴

¹² Stigler and Kindahl's 1970 book provides a classic study on the importance of non-price competition. See also Fisher, 1991, p. 214; Ginsburg, 1993, pp. 94-98; MacLeod, 1995, p. 2; Baker, 2000, section III; and Hartman, 2001, p. 54. For application to high-technology industries, see Pleatsikas and Teece, 2001. And see Krishna and Winston, 2003, who present a theoretical model that supports the "common business school idea that 'the money is at the high end' even when all costs of quality are accounted for." (p. 574)

¹³ The 1995 *Economic Report of the President* noted, "The output of the economy increasingly is shifting away from standardized commodities with easily definable characteristics that change little over time, toward goods and services for which issues of quality and even definition are of primary importance." Leonard Nakamura argues (1997, p. 7) that "buyers prefer higher quality as their incomes rise." Bils and Klenow report (2001, p. 274) ". . . that consumers have been rapidly shifting away from 'static' categories (i.e., those in which there has been little variety or quality gain). . . . Our results suggest that variety has increased by perhaps 1 percent per year over the past 40 years. More striking is that most of this growth occurs in just the past 20 years . . ."

¹⁴ Scheffman and Coleman are similarly optimistic (2003, p. 12).

Perhaps. But let's consider how difficult the problem likely is, both generally and with two specific examples. Thomas B. Leary (2000) outlines no fewer than six types of product differentiation that may be important: 1) product specifications, 2) brand name or reputation, 3) services accompanying the product, 4) "products associated with experiences," 5) "experiences alone," and 6) variety as an independent value. Betancourt and Gautschi (1988, 1990) argue that to fully account for retail services, one must consider five types of retail services: 1) product assortment, both depth (different varieties within a product line) and breadth (different product lines), 2) accessibility of location, 3) ambiance [!], 4) availability of product, and 5) assurance of product availability in the desired form, at the desired time.

Consider two specific examples of the difficulty. Grocery retailers are now competing vigorously to save shoppers' time. In a price-concentration study of grocery retailing one could imagine proxying this form of competition by the number of checkout stands and the number of time-saving services, such as prepared foods, offered. But Coggins and Senauer (1999, pp 170-71) note that it wouldn't be as simple as that: "To make shopping easier, supermarkets are changing their interior designs or floor layouts. . . . In a traditional store it is inconvenient to shop for just a few items or just part of the store. . . . The traditional layout frustrates some customers, however, who find it much easier to shop at convenience stores for such items. . . . Some supermarkets have responded by dramatically redesigning their stores so that the very process of shopping is altered." How would an investigator quantify "store layout"? If it could be quantified, what would it cost to measure? (Coggins and Senauer also note, p. 172, that "Yet another innovation that can have a significant effect on sales is rearranging items on store shelves or improving displays generally.")

Second, economic theory does not demand that business executives be able to measure—or even specify—their marginal costs to the satisfaction of economists. At the leading firm in the office supply superstore market, executives apparently didn't fully understand their marginal cost until recently (Schlosser, 2003):

Within a year of installing Hyperion's Essbase, the finance department realized Staples had been misusing the display space in its stores. The shops had long devoted part of their floor space to desks, file cabinets, and other furniture. It had seemed to make sense: The big products delivered better gross margins than pens and paper. But the BI [business information] system revealed that the strategy was a mistake. 'We found that when you factor in all the costs of storage, distribution, handling, damage, labor, rent/occupancy, etc., the overall profitability of the category is much less than other less bulky, less space-intensive categories,' says [Vice President of Finance] Lerner. 'A desk also takes more labor to sell because you need someone who actually knows a little more about it.'

2. For the sake of argument, let's suppose a diligent investigator is able to identify and measure all the significant non-price factors. But an even bigger problem is created if the analyst includes proxies for cost or quality that the sellers control; the estimated model would no longer be a valid reduced form.

For example, consider store size. Some price-concentration studies in grocery retailing include store size as an independent variable. The rationale is that bigger stores can exploit

economies of scale and can thus operate at lower average cost. The predicted impact on price is therefore negative.¹⁵ But store size is not a fact of Nature. What if firms in more concentrated markets operate larger stores?¹⁶ Part of the net effect of seller concentration on price—part of the effect on consumer welfare that concerns the antitrust authorities—will be misleadingly split off from the coefficient on seller concentration. In a labor-market context, Mark Killingsworth labels (1993, p. 67) this the “included-variable problem.” David Freedman writes (2002, p. 12), “If X and Y cause Z, but X also causes Y, the variable Y would often be treated as an ‘intermediate variable’ along the pathway from X to Z, rather than a confounder. If the object is to estimate the total effect of X on Z, then controlling for Y is usually not advised.” Replace “X” with seller concentration, “Y” with store size or any other cost/quality variable controlled by the firm, and “Z” with price, and his advice applies here.

Quality and cost differences that the firms can affect should not be controlled for by adding variables to a price-concentration model. They shouldn’t be added—at least in a single-equation model estimated by OLS, which is what most price-concentration studies have employed. Doing so obscures the net effect of seller concentration and that is what the investigator is interested in measuring.¹⁷

3. What, then, of including exogenous proxies for cost and quality differences? A variable such as metropolitan-area income? I can’t be too critical of this because I’ve done it myself.¹⁸ I still suggest it as a quick way to see if higher prices in some markets are readily explained by some factor other than market power. Small, statistically insignificant coefficients for seller concentration with such exogenous cost or quality proxies in the equation should be sufficient—though not necessary—to question the presence of market power through coordinated effects.

And Leonard Weiss proposed (1989a, p. 9) adding exogenous proxies for cost differences:

Our typical study will involve some sort of regression that explains price in terms of costs as well as concentration. Aren’t we introducing price-cost margins through the back door? For the most part no. Usually we are not introducing unit costs in an industry but a variable that affects it. We don’t use unit labor costs in cement in determining the costs of a region’s cement plants but average hourly earnings in all manufacturing in that region. . . . We feel that in the typical study we have to use cost, but costs in these forms are so far removed from those actually incurred by the firms involved that they are

¹⁵ There is evidence consistent with this prediction. Roberts and Supina report (1996, p. 920) that for all the industries they examined, except gasoline, “output prices decline with increases in plant size.”

¹⁶ There seems to be evidence of this (Ellickson, 2001).

¹⁷ Note that I am not claiming the bias on the coefficient of seller concentration is necessarily positive. The bias could be negative. For example, an investigator may include unionization of the work force in the equation, reasoning that unionization raises wage rates and that higher wage rates will in turn raise price. But what if more concentrated markets are easier to unionize? Part of the positive net effect of concentration would in that case be split off into the unionization coefficient, so the coefficient on concentration would be biased negatively.

¹⁸ In a price-concentration study of grocery retailing (Newmark, 1990) I argued that income should be included in the estimated equation and in a study of cement (Newmark, 1998) I argued that population density and population should.

consistent with a huge range of profits.

Completely reversing the usual argument, though, Weiss is advising us to find proxies that are *weak*. Weak proxies, proxies “far removed” from the firms’ actual costs, permit the investigator to interpret the estimated regression as a reduced form and allow the investigator to straightforwardly assess the impact of seller concentration on price. But weak proxies will also leave most of the cost and quality differences unexplained. I imagine that you won’t be able to explain the price Starbucks charges for its coffee through metropolitan wage rates. If, on the other hand, the weak proxies are actually strong and highly correlated with firm costs, we have the problem to which Weiss alludes: with a lot of cost-like variables on the right-hand side, we are implicitly measuring a price-cost margin, and that returns us to the fundamental problem of the profit-concentration literature.¹⁹

Adding exogenous proxies for cost and quality is useful in an initial analysis. But they are unlikely to solve, in either a general or powerful way, the problem that non-price competition poses for price-concentration studies.

I close this section by discussing another problem that, while not as serious as the interpretational difficulty, should also remind us of the profit-concentration literature: measuring the dependent variable. In grocery retailing, for example, measuring transaction prices is not necessarily simple. The analyst must contend with coupons, sales, and consumer loyalty programs (Scheffman and Coleman, 2002, p. 361; Kinsey, 1998, p. 12). Scanner data provided by Nielsen or IRI, which investigators without subpoena power rely on, has some notable weaknesses: coverage of mass-merchandisers such as Wal-Mart and of convenience stores is scant; there may be problems incorporating club and loyalty-card discounts; and coverage of promotional activities such as ads, in-store displays, and “shelf-talkers” are “poor to non-existent” (Jacobson, 2003 and Hosken, *et. al.*, 2002, pages 3, 4, and 12).

In producer-goods markets, actual transactions price may be even harder to observe. Robert E. Hall (2001, pp. 4-5) writes:

Businesses go to great lengths to keep their deals secret. It is a common term of sale contracts—put in at the insistence of the seller—that the buyer may not disclose the terms. And when the buyer is a large organization, deals are often structured so that only the top management of the buying company knows the actual terms. It is a common practice to issue invoices at prices above actual prices. The difference is made up in a secret rebate handled only by top management.²⁰

Last, there is the non-trivial problem of combining multiple product prices into a single index. Martin notes this problem (1993, p. 547) and states that because of it, “It is not obvious

¹⁹ On this point see also Scott (1995, p. 22): “When products are heterogeneous, price differences may simply reflect product differences which in turn may cause costs to be different. When cost data are taken into account in an attempt to alleviate this problem (by using price-cost margins), many of the errors associated with using profits data are reintroduced. Once again, there is a fundamental problem with the analysis, which may cause these results to be biased and unreliable as a foundation for policy.”

²⁰ Ginsburg, 1993, p. 98 cites cash discounts in the cement industry. I’ve also seen reported in a court case—I believe it was *Wall Products*, but I haven’t located it yet—that sellers granted significant cash discounts that were not recorded on sales invoices.

that measurement problems in price studies are less serious than measurement problems in profitability studies [footnote omitted].” A specific difficulty is if the index is constructed using a fixed, unweighted set of items, a set that does not reflect the substitutions customers would make patronizing different sellers. Shughart (1998, pp. 37-38) criticized the FTC’s use of such an index in *Staples* and so did Hausman (DeGeorge, 1997, p. 30).

III. Price-concentration studies of industries without non-price competition have a different, but also serious, problem

Suppose a price-concentration study is done on an industry that, despite the discussion above, we believe competes only on price. Say cement, as Weiss, and Scherer and Ross suggested (p. 10 above). Or say office supply superstores. There are still two, closely related problems.

Why does seller concentration differ across the local markets? By assumption, the product and its quality and conditions of sale are the same. Within a given industry the production technology surely must not vary much.

And what if prices differ because costs differ? Many observers have noted that a positive price-concentration relationship might not be due to market power but might simply be due to higher costs in more concentrated markets.²¹ But most of these observers have not provided a theory of why costs tend to be higher in concentrated markets (and these must be costs unrelated to product and service differentiation because we are here assuming such differentiation away).²²

Seller concentration and costs could both be high in markets that are small.

Consider cement. Several studies of the cement industry report a positive correlation between prices and seller concentration. But these studies did not fully account for two significant characteristics of the cement industry. One, cement production has significant economies of scale. And two, cement has a low value relative to its weight, so it is expensive to transport. Together, these characteristics imply that it will be more costly to serve relatively small, isolated markets. When I added proxies for these factors, population density and population, to a price-concentration model for cement, the impact of concentration became much weaker and statistically insignificant (Newmark, 1990).

But what if all markets are large enough to support at least one optimally sized plant and, if there are multiplant economies of scale, at least one firm fully exhausting those economies? As Lambson (1987) and Demsetz (1989) explain, small market size could still be directly responsible for both higher concentration and higher costs. Lambson labels the cause “lumpy technology” and Demsetz labels it “indivisibility” but the underlying concept is the

²¹ See, for example, Kimmel, 1991 (cited in Pautler, 2001, p. 43); Dunne and Roberts, 1992, p. 14; Niskanen, 1997, p. 26; and Simpson and Hosken, 1998, p. 2

²² The exception is Bresnahan, 1989, pp. 1043-44. But his explanation assumes free entry and market equilibrium, assumptions that Evans, Froeb, and Werden criticize (1993, p. 432). The discussion about to be presented does require either condition.

same.

Here's what the concept means. Consider a set of competing firms, N , each producing a single output, q , and each having identical U-shaped average cost curves. Suppose the common average cost curve has a unique minimum point, q_{\min} , and at that quantity, cost is $\$c$. Finally suppose that at a market price of $\$c$, the quantity demanded in the market exceeds q_{\min} times N . What happens? Some demand is not satisfied, so market price rises. Each of the N firms expands output along its rising marginal cost curve. Each firm makes economic profit: each produces an output at which price equal marginal cost, and marginal cost is greater than average cost.

Then, in the standard model of perfect competition, at least one new entrant is attracted into the industry. But what if, Lambson and Demsetz ask, a potential entrant realizes that its entry will drive the market price below $\$c$? The potential entrant, who plans to operate at the average cost minimizing output level q_{\min} , might realize that if q_{\min} were added to the quantity available for sale, market price would fall below $\$c$. All firms, including the entrant, would suffer negative economic profits. The result is that the potential entrant does not enter. The market is in equilibrium at a price that permits incumbent firms to earn economic profits, a price that exceeds average cost.²³

To summarize: if the average cost minimizing output is large relative to the quantity demanded at the minimum average cost, an even number of firms may not "fit" in the market. The output of N firms doesn't satisfy demand, but the output of $N + 1$ firms drives price below the breakeven level. These, then, are the pieces of the theory: 1) lack of fit causes prices to be higher, 2) all else constant, smaller markets will tend to be more affected by lack of fit, and 3) given economies of scale, smaller markets will tend to be more concentrated.²⁴ Therefore, indivisibility could, by itself, create a positive correlation across markets between price and seller concentration. The correlation is not a coordinated effect and it is not a unilateral effect. Significantly, the theory implies that the higher prices of small, concentrated markets do *not* demonstrate that increases in concentration in larger markets will raise price.²⁵

²³ Franklin Fisher does not develop the point as fully as Lambson and Demsetz, but he makes a similar argument (1979, p. 26): "Suppose that the minimum scale which is necessary for efficient production is large relative to demand. In such a case, an incumbent firm may be able to earn monopoly profits because an entrant will properly make the calculation of what profits will be *after* his entry, rather than before, and will perceive that with one more minimum scale firm in the market the addition to supply will be such as to reduce prices below the point where profits cannot be earned."

²⁴ This is not just a theoretical possibility. In Cotterill's 1986 of grocery prices and concentration in 18 Vermont markets, population and the HHI have a negative correlation of $-.54$. (Computed by the author from data in the working paper version of Cotterill's paper, 1984.) In a more recent study of department store prices and concentration, Claycombe, 2000, the correlation between population and four-firm concentration is $-.77$. (Computations again done by this author.)

²⁵ If the merger reduced the number of OSS firms in a market from two to one so the market structure changed to monopoly, wouldn't price have to rise? Doesn't every textbook discussion of monopoly mandate that conclusion? The answer is "not necessarily." One reason: the change to monopoly does not have to raise market price if the monopolist has lower costs than the two original firms did. The *Staples* defendants claimed exactly that, that the merger, itself, would decrease costs. Another reason is that even monopolists can be disciplined by *potential* entry. And a final reason is that our textbook model of monopoly assumes that the market is clearly defined. In virtually all real-world markets, the proper definition of the market is uncertain. In the case of the

What conditions are necessary to apply the theory to the office supply superstore (OSS) firms involved in *Staples*? There must be a factor, or factors, fostering economics of scale such that the average cost minimizing output was “relatively” large. There must be a wide range of market sizes, with some markets being “relatively” small. And there must be higher concentration in smaller markets.

These conditions seem to have been met. At the store level, an important advantage of superstores over traditional office supply stores is that superstores stock, at a minimum, a large number of items, some 5,000 or 6,000. At the firm level, Demsetz argued that an important source of indivisibility is advertising and there is evidence that OSS firms realize advertising economies of scale. Salkir and Warren-Boulton stated (1999, pp. 155-56), “Economies of scale in advertising, for example, clearly appeared at the local and regional levels.”²⁶ The large range of market sizes and relationship between market size and concentration will be discussed below (pp. 22-23).

Let’s look at four pieces of information connected with *Staples* against the background provided by this theory.

1. The FTC employed a compelling example of its case. It noted that “According to Office Depot’s own ads, file folders cost \$1.95 in Orlando, Florida, where it competes with Staples and Office Max, and \$4.17 in Leesburg, Florida, some 50 miles away, where it is the only office supply superstore.”²⁷ This example helped persuade the chairman of the FTC, Robert Pitofsky, to proceed with the FTC’s challenge.²⁸ In 1990, the population of Leesburg was 14,903, while Orlando’s population was 164,693.²⁹

2. Crucial to the FTC’s case was data indicating that in markets where Staples was the only OSS firm, prices were higher than in markets where it competed against Office Depot and Office Max. Prices in Staples-only markets were 13% higher than in three-firm markets. Similarly, prices in Office Depot-only markets were 5% higher than in three-firm markets.³⁰

Hot documents indicated this pattern was no coincidence. Both Staples and Office Depot set prices according to the number of OSS firms present in the market. One Office Depot memo stated (Singer, 1997), “We have one pricing zone in our company that is made up of stores that do not have any superstore competition. . . . This zone contains the highest-priced stores in our company, as these stores do not have any competition.”³¹ A Staples document defined “competitive” markets as markets with another OSS firm and

office supply superstore firms of *Staples*, a “monopolist” that tried to exploit its market power might well induce competition from other vendors of office supplies.

²⁶ A further indication that there were probably substantial multi-store economies of scale in this industry is that the number of OSS firms has decreased nationally, in only few years, from twenty-three to just three. See *Federal Trade Commission v. Staples, Inc. and Office Depot, Inc.*, pp. 39-40.

²⁷ U. S. Federal Trade Commission, “FTC Rejects Proposed Settlement In Staples/Office Depot Merger.” April 4, 1997, p. 1.

²⁸ U. S. Federal Trade Commission, “Plaintiff’s Memorandum,” April 10, 1997, p. 9, and Singer, 1997

²⁹ I obtained 1990 Census population figures from <http://venus.census.gov/cdrom/lookup>, database STF3A. 1990 was a few years after the period covered by the case, but that seems highly unlikely to affect the comparison materially.

³⁰ *Federal Trade Commission v. Staples, Inc. and Office Depot, Inc.*, pp. 15-16.

³¹ Singer, 1997.

“noncompetitive” markets as markets without another OSS firm (Dalkir and Warren-Boulton, 1999, p. 148).

But a business executive’s use of “noncompetitive” does not have to match an economist’s use of the word. If “noncompetitive” markets tended to be the smallest markets, the indivisibility theory predicts higher prices in them, higher prices having no connotation of lack of competition in the economic sense.

3. The defendants argued that, in fact, costs were higher in one-firm OSS markets. They cited a “myriad of other factors” including variations in sales volume, product mix, real estate and advertising costs, wages, a store’s distance from regional distribution centers, demographics, and the higher costs of doing business in rural areas.³² But they failed to present a simple, appealing theory of why costs should be generally higher in *concentrated* markets. They also couldn’t quantify these higher costs to the trial judge’s satisfaction.

The FTC also looked very carefully for cost differences—FTC economists considered costs beyond those the defense formally presented at trial—but didn’t find any costs that could explain the higher prices. Baker wrote (1997, p. 8), “Our extensive review turned up no evidence of important unobservable cost variables affecting pricing, except in one city.”

Well, what about that? What costs, specifically, were higher? Why couldn’t the FTC find any?

I don’t claim to offer a complete answer to these questions. But I can make one general comment about measuring costs and three comments based on the indivisibility theory.

--The profit-concentration literature discussed for years how extracting economic costs from accounting data is difficult. Determining marginal marketing, warehousing, information-systems, and transportation costs for office supply superstores should be similarly challenging. Even a cost apparently easy to measure—labor expense—can involve difficulties. For example, Dunne and Roberts used data at the plant level to estimate a reduced-form equation for the price of bread. They found that production workers’ wages were not significant, while non-production workers’ wages were significant but *negatively* related to price. They decided that the most likely explanation for the second result was that wage rates for non-production workers reflected differences in worker quality (1992, pp. 23-25).

--In markets with a lack of “fit,” indivisibility predicts price will exceed average cost, perhaps significantly. Only if investigators compute marginal cost would cost fully “explain” higher prices, but as just noted marginal cost can be difficult to measure.

³² Defendant documents cited in Grengs, p. 31. The reference to variations in product mix is interesting and suggests that the non-price competition story of part II may also have played a role in the price differences. While office supply superstores seem to be very similar across markets, there were some differences that were not explored much in *Staples*. The fraction of store sales accounted for by general office supplies varied across markets: in one-firm markets, the average was 26.68%; in three-firm markets, 37.29%. The fraction due to computer sales, on the other hand, was highest in Staples-only markets and lowest in three-firm markets. Also interesting is the variation in advertising expenditures. Both prices and per-store advertising expenses were higher in Staples/Office Max areas than in Staples/Office Depot areas (*Federal Trade Commission v. Staples, Inc. and Office Depot, Inc.*, p. 36).

--The theory implies that lack of fit, and hence higher prices, will not be a simple linear function of market size. Suppose an average cost minimizing store can serve 100,000 people. If the market has 200,000 people, two stores perfectly fit. If the two stores belong to different firms who compete vigorously, price will approach minimum average cost. But if the market has 150,000 people or 250,000 people, two stores will not fit perfectly and holding the degree of competitiveness constant, price should be higher than in the 200,000-person city. This nonlinearity complicates measuring the effect of indivisibility.

--Smaller markets might well be more expensive to enter and/or riskier than larger markets. More expensive because if executives act as if they know above-normal profits can be earned from lack of fit, there should be intense, costly competition to be first to enter smaller markets (Demsetz, 1995, p. 147). That there is possibly more risk is suggested by two things. Part of the conventional wisdom about Wal-Mart's success is that they exploited a crucial error made by Sears and the other general merchandise chains. Those chains could have entered the small, mostly Southern towns where Wal-Mart got its start. But, the story goes, Sears and the other chains thought that rural towns might not support large chain stores, hence entering them would be too risky. Another possible source of risk for a firm entering a small market is the possibility that one of its competitors might overestimate the market's potential. Over-entry into a small market could drive price further below the breakeven level than over-entry into a large market. Measuring either of these costs would be challenging, to say the least. But the hypothesis of greater risk in smaller markets might be testable by looking at the geographic pattern of store closings and firm exits.

4. A necessary condition for the theory to apply to Staples is that the one-firm OSS markets were smaller than two- and three-firm markets. Was that true? It seems so. An expert witness testified that one-firm markets were "typically" smaller.³³ In one of its filings the FTC listed 15 markets as having two firms and another 27 markets as having three firms. I computed the median population of the two-firm markets as 358,360 and the median population of the three-firm markets as 1,136,607. Only five of the fifteen two-firm markets—Baltimore, Louisville, San Diego, Tampa-St. Petersburg, and Washington, D.C.—had a population greater than 500,000, while only six of the twenty-seven three-firm markets had a population smaller than 500,000.³⁴

Data not presented in the court proceedings, summarized in the table below, also indicate that one-firm markets were smaller.³⁵ The median population of metropolitan areas with only one OSS firm was 225,498. (Staples-only areas had a median population even

³³ *Federal Trade Commission v. Staples, Inc. and Office Depot, Inc.*, p. 35.

³⁴ The cities are listed in U. S. Federal Trade Commission, "Plaintiff's Memorandum," April 10, 1997, footnotes 23 and 24. I obtained the metropolitan area populations from data produced by the U. S. Bureau of Economic Analysis's "Regional Economic Information System" accessible at <http://fisher.lib.virginia.edu/reis/index.html>. (Upon checking this Web link on 2/10/04, I found it no longer functions. In a future revision of this paper, I'll find a new source for this data.)

³⁵ These data were generously provided to the author by Dr. Serdar Dalkir of MICRA, Inc. Dr. Dalkir collected the data for a personal research project after the *Staples* case ended. He is not at all responsible for any of the computations underlying the table or for the conclusion drawn from it.

smaller, 170,808.) Metropolitan areas with two OSS firms had a median population of 612,455. Metro areas with three OSS firms had a median population of 1,509,712.³⁶

Median Population of U.S. Metro Areas, Grouped by OSS Firms Present

Staples Only	170,808
Office Depot Only	220,837
Office Max Only	257,492
Staples and Office Depot	858,113
Staples and Office Max	614,510
Office Depot and Office Max	537,005
All One-Firm Areas	225,498
All Two-Firm Areas	612,455
All Three-Firm Areas	1,509,172
Areas with No OSS Firms	120,897

But based on the documents that are in the public domain, it doesn't appear that either the FTC or the defendants' economic expert, Jerry Hausman, considered the lumpy technology/indivisibility theory. It also appears that the parties did not consider market size, at least not directly, in their empirical work.³⁷

What Jerry Hausman did do—when the FTC apparently would have been happy to rest on the positive correlation between prices and concentration in cross-section—is force the FTC to consider the possibility that there might be unmeasured costs of some type that were correlated with concentration (Baker, 1997, p. 5). As Hausman and Leonard put it (p. 2): “. . . the FTC's approach based on the documents assumes that the merger would turn a two OSS area like Washington, DC into a one OSS area like Middletown, NY. Yet, Washington and Middletown differ in many ways besides the number of OSS chains, making this assumption questionable.”

³⁶ Dr. Salkir's spreadsheet lists 1391 OSS stores and provides each store's city, state, and zip code. This information, used with the software program Zip Express (<http://www.getzips.com/download.htm>), allowed me to identify the county in which each store was located. Counties were linked to metropolitan area definitions. Finally, metro area populations (for 7/1/97) were obtained from Census data at <http://eire.census.gov/popest/archives/metro/ma99-03b.txt>.

³⁷ The preceding two statements are made with qualifications. Baker writes (1997, footnote 26) that the FTC included in its econometric model variables “accounting for exogenous determinants of cost and demand (such as paper prices and ‘fixed effect’ indicator variables for each sample period).” Perhaps market size was included as one such variable, but I know of no publicly available document that reveals this. Baker also states (footnote 47) that the FTC used market size in a supplementary statistical analysis. The defense had argued that entry of OSS firms into a particular market might be endogenous: high prices might encourage entry. So the FTC estimated a second equation which specified that the number of Staples and Office Depot stores in a market was a function of market size and other variables. They then used that equation to construct an instrumental variable for the number of stores in the price model. But in that use, market size does not seem to address indivisibility either theoretically or empirically. Theoretically, indivisibility implies that entry may not be strongly related to price. Rather than price inducing entry, both price and entry are functions of indivisibility. And as noted above, indivisibility will be a nonlinear function of market size. Empirically, even if market size is used as an instrumental variable, it will not control for indivisibility unless it also appears as an independent variable in the price equation.

To control for unmeasured cost differences across markets, Hausman pooled the cross-section data with time series data and estimated a “fixed effects” model. The FTC employed Orley Ashenfelter to follow the same approach. In a fixed effects model a dummy variable is assigned to each geographic market. Fixed cost differences across markets, including those caused by market size, are thus statistically controlled; therefore, “the estimated effect of Office Depot rivalry on Staples pricing comes solely from pricing variation within markets over time.”³⁸

Do fixed effects solve any problem caused by differing market sizes, by indivisibility? I think four questions remain.

1. Market concentration is still endogenous. The fixed effects model does not address why some markets are more concentrated than others.³⁹ Even more importantly, it does not explain or allow for the fact that entry is occurring in some markets but not others. Suppose we find that entry occurs mostly in larger markets. That would be puzzling since the hot documents describe one-firm OSS markets as more profitable, and as discussed above, the one-firm markets seem to be the smaller markets. On the other hand, suppose we find that entry occurs mostly in these smaller, “noncompetitive” markets. If the indivisibility story applies, prices in smaller markets have further to fall simply because they are small: a prediction of the price response to entry in large markets based on the response observed in small markets would be misleading.

2. It would be nice to know how many times entry occurred and if there were a few entries that had a disproportionately large impact. Studies dealing with small, non-randomly selected samples sometimes produce impressive t-statistics that are driven by just a small number of observations. Careful scrutiny of those few observations can then uncover a completely different explanation of what is occurring. (For two examples, see Newmark, 1988 and Newmark, 1989.)

3. The fixed-effects model uses the observed effect of entry to forecast the effect of a horizontal merger. Hausman and Leonard summarize the procedure as follows (p. 2): “Having estimated the effect of adding a second OSS chain to a one OSS area using the actual occurrences of such additions, we predicted the effect of the merger by essentially reversing the process (we turned the two OSS areas back into the same area less one OSS chain).”

But reversing the process of entry does not compare exactly to a merger. When entry occurs, total output for sale in the market should go up (though one does have to consider strategic responses by incumbent firms). As supply rises, market price should fall.⁴⁰ But a merger does not necessarily change the amount of output for sale; initially, it just changes the ownership of that output. Whinston makes essentially the same point (2003, p. 29):

³⁸ Baker, 1997, p. 6. See also Baker and Rubinfeld (1999, p. 401): “Because the unobservable costs were unlikely to vary over an 18 month period in any one location, the model with store fixed effects accounts for the possibility that what appears to be the effect of market structure on price should actually be attributed to unobservable cost variation.”

³⁹ Janusz Ordover makes a closely related point (U. S. Federal Trade Commission, 2001, pp. 60-61) (though not in the context of *Staples*): the investigator should ask why are mergers occurring in some markets and not others?

⁴⁰ Consistent with that prediction, defendants argued that when Staples entered a market without other OSS firms, prices dropped 25% (Grengs, p. 31).

. . . consider the UP/SP [merger of Union Pacific/Southern Pacific] example. One factor that is probably important for the determination of prices on a route is the level of aggregate capacity available on that route (such as tracks, sidings, and yards); higher capacity is likely to lead to lower prices, all else equal. In the pre-merger data, this aggregate capacity level is likely to be correlated with the number and identity of competitors on a route. For example, aggregate capacity is probably larger when more firms are present. Hence, in a regression that includes the number of firms on a route, but not capacity, some of the effect that is attributed to an increase in concentration is likely to be due instead to the fact that across the population of markets, higher concentration is correlated with lower capacity levels. But in a merger, while the number of firms will decrease on many routes, the level of capacity on these routes may well remain unchanged (at least in the short-run). If so, the regression would predict too large an elevation in price following the merger.

4. Recent research raises a last complication to evaluating horizontal mergers using time series data: any efficiency effects of the mergers may take time to develop. Morrison states (1996) that some studies evaluating airline mergers “have been limited to tracing the effects for about a year after the mergers” and he argues that investigators need to examine longer time periods, periods long enough for the changes induced by a merger “to work themselves out fully.” When he does so, he finds results that “show these [two] mergers in a much more favourable light than the two short-run studies.” Focarelli and Panetta argue (2003, p. 1153) that any increase in market power from a merger can show up almost immediately but any increased efficiency may take some time to become evident. Why? They say cost-cutting takes time; merging corporate cultures successfully takes time. In their study of Italian banks, they find support for their argument.^{41, 42}

I close this section by noting that the effect of market size on price-concentration studies may be intensified by another problem: the geographic markets examined are dictated by data availability. Almarin Phillips conjectured that this might be distorting the results of an influential price-concentration study, the granddaddy of current studies (U. S. Congress, Joint Economic Committee, 1974):

SMSA population sizes are not normally distributed. While we do not know which 36 areas are covered in the [Joint Economic Committee] study, the full SMSA size distribution is strongly skewed, with relatively few large cities and increasing frequency by class as SMSA size falls. The regressions, then, are based on one or more extreme values for size—and anything associated with size, perhaps, as concentration—and a cluster of the more common values. The

⁴¹ The effect may go the opposite way, too. Gallet (2001) argues that “mergers gradually increased market power” in the U. S. steel industry.

⁴² Baker presents another criticism of Hausman’s model (1997, footnote 33). He argues that Hausman’s procedure is susceptible to error because the exact date the entrant affects market price is uncertain: the incumbent firm might lower price before actual entry occurs. (Imagine the incumbent having a sale right before the other firm enters; the sale is intended to reduce the number of customers who will sample the new firm.) If so, market price might *rise* after entry, prompting an incorrect interpretation of the effect of the merger. What’s not clear to me yet is why the FTC’s own fixed effects model isn’t subject to the same criticism.

OLS results are dominated by the few extreme values and, essentially, the means of the cluster. (1980, p. 288)

Since the JEC data were collected confidentially, full details of the cities used are not publicly available. But information published subsequently (Geithman, Marvel, and Weiss, 1981) suggests that Phillips was right. A diagram shows (p. 351; see also table, p. 352) that for four-firm concentration ratios between 40 and 60 percent, there is virtually no relationship between price and concentration. The significant, positive price-concentration relationship in the JEC data seems to have been driven by just five markets, three low-price markets with concentration ratios below 40 percent and two very high-price markets that had concentration ratios above 75 percent. One of the two high-priced cities is known to be Washington, D.C. Washington, considering the metropolitan area as a whole, had significantly above-average income, and income is not included in the JEC regression; non-price competition could be responsible for at least that one observation.⁴³

IV. Some suggestions for doing price-concentration studies and for further research

Based on the discussion above, I offer three easily implemented suggestions for improving price-concentration studies.

1. Always include a measure of consumer income and always include a measure of market size.
2. Don't include endogenous measures or proxies of firms' costs.
3. Ideally, one should have a large sample of markets, a sample that includes many markets of both low concentration and high concentration. If that is not possible, take extra care to examine the markets having extreme values.

I also have two suggestions for further research.

First, it would help if we better understood what determines seller concentration. Economists have an incomplete understanding of what determines interindustry differences in the level and change of concentration. Beyond measures of economies of scale and capital intensity, empirical work doesn't seem to find impressive results.⁴⁴ Explaining intraindustry difference seems even more challenging. If demand and supply conditions are similar across markets within a single industry, why are there large, long-lived differences in seller concentration? Why, for example, was seller concentration in grocery retailing high for many years in Denver—from 1954 to 1982 it had a four-firm concentration ratio ranging from .67 to .86—but much lower in, say, Minneapolis (.31 to .44)? Why are the six most highly concentrated grocery retailing markets in the U. S., currently—and 8 of the 15 most highly

⁴³ That Washington is one of the sampled markets is stated at JEC, 1974, p. 35.

⁴⁴ See, for examples, Curry and George (1983) and Levy (1985). Recent work seeking to test John Sutton's model, such as Ellickson (2001) and Astrid Dick (2001, 2002), promise a better understanding of selling concentration, however.

concentrated, and 12 of the 26 most highly concentrated—located in Texas? Why, on the other hand, are 6 of the 16 least concentrated markets located in New Jersey?⁴⁵

Some of the explanation may lie in detailed information about local areas, as suggested in an article about the poor state of supermarkets in the Northeast (Collins, 1995, p. D-1):

Some of the differences between Grand Union and Publix can be attributed to the difficulty of doing business in the Northeast, and can help to explain the reasons that high-quality supermarket companies from other regions have not entered this market. In the Northeast, there are 'more older supermarkets in constricted spaces with poor parking and poor convenience,' said Mark Husson, a retail analyst at J. P. Morgan Securities.

In the South, 'you can build a 50,000 square-foot store in six months in an area where there will be potential for future growth,' said a spokesman for the A. & P. chain, Michael J. Rourke. But in suburban New York, building a similar store can take four years, because of zoning, environmental and traffic considerations.

And some of the explanation probably is, as suggested above, a matter of market size. In any event, more research on the determinants of concentration would improve the inferences we draw from price-concentration studies.

Second, if price data disaggregated by firm is available and if at least some markets are not too highly concentrated, Demsetz's test for profit-concentration data could be applied to price-concentration data. Specifically, if prices are higher in more concentrated markets, are all firms in the concentrated markets charging higher prices, or are just the leading (larger market-share) firms? If just the leading firms are able to charge higher prices, it's possible that competitive superiority rather than coordinated effects is responsible for the price-concentration correlation.

I use Cotterill's 1986 study to illustrate. In a specification that does not include firm market share, Cotterill reports (p. 385) that the four-firm concentration ratio is positively and significantly related to grocery prices (t-ratio of 2.7). But when he adds firm market share divided by concentration to this specification—given that four-firm concentration equals 100% in 14 of the 18 markets in the study, and has a minimum value of 80%, this variable is close to being just market share—the coefficient of concentration declines by nearly one-half and its t-ratio declines to 1.5. The (almost) firm market share is positive and significant (t-ratio = 3.0).⁴⁶

I've proposed this approach before and Werden objected to it (1991, p. 8):

⁴⁵ Author's preliminary calculations based on *Market Scope* data provided courtesy of Phil Kaufman at the U. S. Department of Agriculture. Dr. Kaufman is not responsible for the calculations or conclusions expressed here.

⁴⁶ Wen notes (2001, p. 37) that Cotterill did not report a specification including both concentration and market share in his more recent 1999 study: "However, when replacing concentration with individual market shares of the supermarkets, he finds that this is also statistically significant and positively related to price. . . . For unexplained reasons, the author does not report regressions where both the concentration ratio and the relative market share of firms are explanatory variables, which would allow the researcher to potentially discriminate between alternative sources of market power, even though he advocates this in Cotterill (1993)."

Instead, he [Newmark] suggested that firms rather than markets be used as observations and that market share and concentration both be used as explanatory variables. The effect of market share on price would be attributed to some firm-specific superiority, while the effect of concentration would be attributed to market power. This is a dubious interpretation because a positive association between price and market share certainly can be the effect of market power.

To which I reply, sure, a positive correlation between high market share and price could be the result of a unilateral effect. But the exact same point can be made, and has been made, regarding the effect of market share on profits. That is, in regressions of profits on both concentration and market share, concentration has an effect not significantly different from zero, but market share typically has a large and significant positive effect. Such an effect could well be ascribed to unilateral market power (see, for example, Martin 1998), yet this finding marked the end of the antitrust community's interest in profit-concentration studies (Weiss, 1989a, p. 8). Why?

I believe it is because findings based on high concentration once offered the promise of simple, bright-line rules. Findings based on high market shares offer much less guidance for antitrust policy. A firm charging higher prices than its competitors and making apparently large profits—Starbucks, or Intel, or dare I say, Microsoft—could be exploiting market power to the detriment of consumer welfare. But, as I've tried to indicate above, it could also be out-competing its rivals to the great benefit of consumers. Thus, while adding firm market share to a price-concentration model would not end the analysis, to the extent it provided additional information on the coordinated-effects hypothesis, it would be useful.

V. Conclusion

None of the foregoing should be read as argument against using specific Herfindahl numbers in the Merger Guidelines. I agree with the many scholars who argue that the benefits of clear and simple rules, particularly if relied on in just the first stage of evaluating a merger, far outweigh their costs (Hay and Werden, 1993, p. 176; Schmalensee, 1987, p. 49; Kolasky, 2002b, p. 8; Baker and Salop, p. 10).

What I am arguing is that price-concentration studies currently do very little to support the “structural presumption,” let alone support the specific HHI cutoffs currently employed. I am also arguing that price-concentration studies, even when employed in the latter stages of a merger investigation, must be interpreted with great care at the least and probably should not be employed at all.

How they have been used has, and will probably continue to have, significant consequences. For an example, consider once again grocery retailing. There seems to be significant pressure toward consolidation. The national market share of the top five U.S. supermarket chains had been, for many years, about 27%. During the second half of the 1990s, this share increased, mostly through “frantic merger and acquisition activity” to 43%

(Wrigley, 2002, p. 63). Wrigley argues that this consolidation had been long overdue and an important reason for its delay was strict antitrust scrutiny (Wrigley, 1992 and 2002).

But antitrust enforcement of grocery retailing has also been rising recently. David Balto noted (1999, p. 2), “Yet in the last four years the FTC has taken more than ten enforcement actions involving supermarket mergers, more than any other industry except pharmaceuticals . . .” Concentration is high enough in many local markets that horizontal mergers will receive the strictest scrutiny. For 1987, for the 270 metropolitan areas the Census Bureau could provide exact supermarket Herfindahls, the average HHI was 2254; 97 areas out of the 270 had HHIs above 2500 (Franklin and Cotterill, 1993, p. 29). Using more recent but less precise data, I estimate the average supermarket HHI over 316 metropolitan areas to be 2286, with 100 areas having values over 2500. Based on the information released by the FTC (2004), it seems as though the median pre-merger HHI for grocery markets involved in recent merger challenges, is in the range of 2300 to 3200.⁴⁷ Considered together, these numbers suggest that if grocery retailers want to continue to consolidate, they will be frequently challenged by the FTC.⁴⁸

Given that several price-concentration studies in grocery retailing have reported significant positive relationships between price and concentration (most recently, Cotterill 1999), and given that these findings seem to be widely seen as useful for antitrust policy,⁴⁹ and given that supermarkets—with all that detailed scanner and loyalty-card data—would seem to be a ripe target for more such studies, I would simply argue for lots of caution. Reliance on such studies could encourage the agencies to block mergers that are not harmful.

Price-concentration studies are one of the important new ideas in antitrust economics in the last fifteen years. We would do well to remember Hay and Werden’s warning (1993, p. 177): “One clear message from history is that new ideas and results ultimately may have little policy relevance.”

⁴⁷ The median post-merger HHI seems to be in the upper part of the 3000 to 4000 range (call the point estimate 3500); the median change in the HHI falls in the 800 to 1200 range. So these number would imply, roughly, that the median pre-merger HHI was somewhere between 2300 and 3200.

⁴⁸ Werden notes (2001, pp. 62-63) that over the last fifteen years many mergers producing post-merger HHIs in the “highly concentrated” range have gone unchallenged and that the more important determinant of government challenges is the change in the HHI; he notes that very few challenges have alleged a change in the HHI of less than 500. This granted, it still seems that in a significant fraction of metropolitan markets, any sizeable horizontal merger will likely be challenged, especially given the recent history of challenges in this industry.

⁴⁹ See, for examples, Balto, 1999, p. 12 and Sexton and Zhang, 2000, p. 18.

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